

FIEF Working Paper Series 2002



No. 183

Coping with Heterogeneities in the Difference in Differences Design

by

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Abstract

Carling, Holmlund and Vejsiu reported in the October 2001 issue of the *Economic Journal* that a cut in the unemployment insurance replacement rate from 80 to 75 percent caused a 10 percent increase in the job finding rate. They also identify an anticipatory reform effect up to five months before the reform. Implied elasticity at 1,6 is high compared to previous research in Sweden and elsewhere. After analysing their data for various heterogeneities, we conclude that the estimated coefficients designed to capture the reform and the anticipatory effects are statistical artefacts. The standard statistical tests and analytic procedures used by CHV do not warn for this eventuality.

Keywords: Heterogeneities; Difference in differences design; Unemployment insurance; Unemployment compensation

JEL classification: J64, J65

December 16, 2002

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1. Introduction

In April 1995 the Swedish Government announced a reform package to tackle the budget deficit. Among the measures taken, was a cut in the replacement rate of the unemployment insurance from 80 to 75 percent, effective as of January 1, 1996. Kenneth Carling, Bertil Holmlund and Altin Vejsiu (CHV) report in the *Economic Journal* (vol 111, No 474) that the cut caused a 10 percent increase in the job finding rate among the insured, implying an elasticity as high as 1,6 per cent. They also report that the reform began to affect the behavior of the unemployed already several months before its actual implementation in January 1996. Could such a small change, economically trivial to most affected households, cause such changes in behavior among the unemployed?

In this paper we analyze the CHV data, successfully replicating their models.¹ We highlight the fundamental heterogeneity of labour markets and labour market behavior that is hidden in their concept of “transition to regular jobs”. Their statistical design focusing on the quasi-experimental difference-in-differences does not cope effectively with this heterogeneity. Their finding of a ten per cent jump in job findings turns out to be a statistical artifact resulting from these heterogeneities. Their finding of statistically significant coefficients for the “reform effect” several months *before* the reform is actually an indication that the coefficient has nothing to do with the reform.

2. Replicating the CHV models

Using the data obtained from CHV we were able to reproduce the sample characteristics in their *Table 1* for the variables in our model, the distribution of replacement rates before 1996 in their *Table 2* and the distribution of durations in their *Table 3* with trivial differences only.

Our model estimates are reported in tables A1 and A2 in the appendix as obtained for the Cox proportional hazards model and according partial likelihood estimation (the PHREG procedure in SAS). Our reform effect is a bit higher, 0.15, compared to 0.12 in CHV. As alternatives, which are

¹ We have presented our reanalysis of the CBV study of the 1996 change in the compensation level in Johansson & Selen (2000). In Johansson & Selén (2001) we study the 1993 cut in benefits from 90 to 80 percent using a similar design, statistical method and type of administrative data. We find heterogeneity causing the same type of statistical problems as our reanalysis identifies in the CHV study.

closer to the specification of CHV, a piecewise constant exponential model as well as a model in discrete time with a baseline hazard estimated for time periods of four weeks have been estimated with the help of TDA.² The differences between the estimates of the reform effect in our three alternatives are negligible.

The choice of explanatory variables in our model is equal to the third model of CHV. With few exceptions, estimates are close to their estimates for the combined exit codes (column 3, *Table 4*). The notable exception is that the labour market indicator, the local unemployment rate by month, is important according to our estimation. Our coefficient is -3.39 and strongly significant, while CHV obtained $-.694$ with a standard error of 0.399 . Their anomalous result that the local unemployment rate has no effect on the job finding rate intrigued us. Searching for specifications that might replicate their result, an estimate of about the same size $-.65$ was obtained when the unemployment rate was fixed in time (instead of time variant) and equal to the rate of the last month in the unemployment period.

Larger differences in estimating effects are obtained for the procedure used for examining anticipation effects by CHV. We have not been able to explain those differences.

3. The hidden heterogeneity of “regular jobs”

Administrative records many times provide excellent data for research particularly when they contain data on economic transactions, which are controlled and audited as are the sums paid for the unemployment insurance. However, administrative definitions are just that and must be checked against the theoretical concepts before they are used in research. CHV imply that their “transitions to work” from unemployment are transitions to “regular jobs”. However, they merge four administrative codes used by the labor exchange offices for “regular jobs” with a “code 11” for “Other transitions to work”. This code brings together a variety of other codes that are not really “regular” jobs:

- 21 = transition to part-time unemployment with the insurance compensating to full time;
- 31 = transition to (very) temporary work;
- 41 = transition to employment for disabled with a wage subsidy;
- 42 = transition to public sheltered employment.

² See Blossfeld and Rohwer (1995).

These “11-coded” individuals constitute no less than 40 per cent of all transitions to work and are, as we shall see, crucially important for the reform effect to appear. If this category is not classified among the transitions to work, there is no reform effect.

4. The heterogeneity of labour markets and behaviour

The five main codes differentiate very different categories of unemployed that have been differently affected by the changes in the labour market during the years before and after the reform. Gender and age are pertinent to labour market behaviour. This is documented in *Table 1* with estimates for transitions to work by control variables, which are excerpted from the full model in *Table A1* in the appendix.

In column 1 we compare differences in transition rates in the after period with the before period for the different exit codes. There are much fewer transitions to “regular permanent jobs” among the unemployed in the sample after the reform was effective from January 1, 1996 according to the estimate (–.430) in column 1. This seems to be true also for the “6-coded” (–.364), who presumably found jobs on their own. The chances for regular temporary employment did not decline, nor the chances to be reem

Table 1. Estimates for control variables in separate models for each of the registered transitions to “work” included as “regular employment” by CHV.

| Code | Type of transition | After reform | T1-group | T2-group | Gender: women | Age |
|----------------|----------------------------------|--------------|----------|----------|---------------|----------|
| 1 | Regular permanent employment | -.430*** | -.366*** | -.030 | -.650*** | -.012*** |
| 2 | Regular temporary employment | -.021 | -.205 | +.052 | -.547*** | -.000 |
| 3 | Re-employed by previous employer | -.079 | +.027 | +.352*** | -1.183*** | +.004 |
| 6 | Kind of employment not known | -.364*** | -.188 | -.216*** | -.454*** | -.035*** |
| 11 | Other transitions to “work” | +.184** | +.180** | +.061 | +.355*** | -.005** |
| 1+2+3 +6+11 | All the transitions | -.184*** | -.157*** | +.009 | -.240*** | -.008*** |

*** 01 level significant, ** 05 level significant. For number of transitions see *Table 2*.

ployed by the previous employer. The coefficients are not statistically significant but also very low (-.021 and -.079 respectively). Other transitions to “work” (code 11) *increased* after the reform (+.184).

The next two columns document the fairly sizable differences between the two treatment groups in comparisons with the control group for the different exit codes. The T1-group includes the unemployed who were exposed to the full impact of the 5-percentage point benefit cut and the T2-group the unemployed, who were affected but by less than 5-percent cuts. From CHV and our reanalysis we know that the T1-group consists mainly of lower paid women laid off from primarily public sector work and that the control group consists mainly of higher paid men with incomes above the ceiling in the unemployment insurance and are mainly from the private sector. The T1-group much less often found regular permanent jobs than the control group (-.366) but made other transitions to “work” more often (+.180).

In the gender column we see that women throughout the period had slim chances, in comparison to men, of getting regular permanent employment (-.650), regular temporary employment (-.547), being re-employed by their previous employer (-1.183) or finding a job on their own (-.454), while their chances of ending up in other transitions to “work” were far greater than that of men (+.355). Finally, age does not appear to be an asset when it comes to finding employment.

The last row in *Table 1* shows the coefficients from the model with merged exit codes, in which the fundamental heterogeneity of labour markets and labour market behaviour is hidden.

5. The non-effect of benefit change

The columns in tables A1 and A2 show estimates for different exit codes treating episodes with other codes as right-censored. Thus each analysis is conditional on the individuals being precluded from the different alternative exits. While an elimination of possible exits in reality will probably affect the behavior of individuals, the purpose of the exercise here is to examine the effects on the result of gradually considering other exits than “*regular permanent jobs*”. In *Table 2* we report the coefficients for the reform effect together with the p-values of the estimates excerpted from tables A1 and A2 in the appendix. Recall that the coefficient measures the difference-in-differences in job finding rate before and after January 1, 1996 between the two treatment groups combined and the control group.

Table 2. Estimated reform effect in separate models for each of the registered transitions to "work" included as "regular employment" by CHV-team.

| Code | Type of transition from unemployment to "work" | Reform effect DPOL | p-values | Number of transitions |
|----------------|---|--------------------|----------|-----------------------|
| 1 | To regular permanent employment | -.006 | .947 | 3 357 |
| 2 | To regular temporary employment | +.033 | .836 | 826 |
| 3 | Re-employed by previous employer | -.187 | .372 | 711 |
| 6 | To kind of job not known | +.233 | .183 | 814 |
| 11 | To other types of "work" | +.027 | .754 | 3 720 |
| 1+2+3 +6+11 | All the transitions | +.150 | .004 | 9 428 |
| 1+2+3 +6 | To regular permanent and temporary jobs | +.080 | .230 | 5 708 |
| 1+11 | To regular permanent jobs and to other types of "work". | +.173 | .004 | 7 077 |

Note first that the coefficients for the "reform effect" for the five separate types of transitions have different signs and none is even close to statistical significance! However, when we run a model with all five "transitions to work" categories combined, we get a coefficient for the "reform effect" that has the correct estimated sign, i.e. positive and significant at the one per cent level.

When we remove code 11 from the combined transition categories, there is no statistically significant reform effect. Removing them would be for substantive reasons. Code 11 does not really represent "transitions to regular jobs" but for the most part to labour market programs. The benefit cut did not increase transitions to regular jobs.

Note next that there is no reform effect for those in code 11 when modelled separately. It is the combination of code 1 and 11 that produces the significant coefficient for the "reform effect". The two codes represent totally different categories in terms of demographic and socio-economic composition and labour market situations. The statistically significant coefficient that appears when these different categories are merged, has nothing to do with the reform but appears because of the merging of the two very different categories in terms of labour market situation.

We conclude that the coefficient reported by CHV, is a statistical artifact that does not lend itself to causal interpretation.

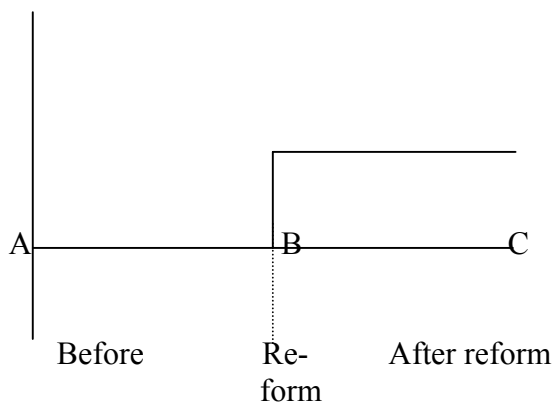
6. The anticipatory effect

Our conclusion that the estimated coefficient for the reform effect is a statistical artefact is further supported when we analyse the anticipatory effect that CHV have identified.

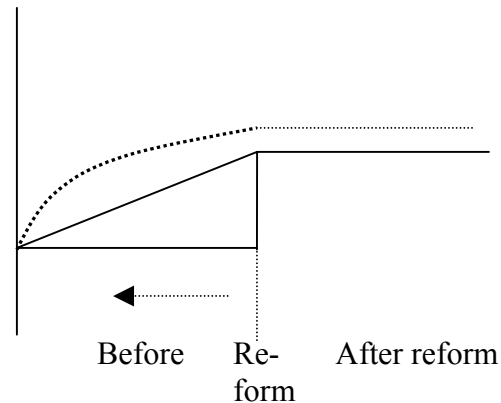
CHV provide a neat theoretical explanation for an anticipatory effect. A cut in the compensation rate theoretically functions as an insurance system with two compensation levels, an initially high level followed by a lower level after some period of unemployment. The optimal adaptation to such a system for the unemployed is to let the reservation wage go down until the reform is implemented after which the reservation wage is constant. The job finding rate thus increases until the reform goes into effect and is constant after that.

CHV are aware of one objection; how one can logically have both an anticipatory effect and simultaneously a reform effect when comparing before and after. They provide a brief answer to this objection that it is possible that the “before/after” effect (with an already high elasticity of 1.6) is an underestimate (!) of the true effect.³ Let us now take a closer look at their method of measuring the anticipatory effect.

Figure 1. Simple reform effect immediately after reform implementation.



Figur 2. Reform effect with anticipatory effect and thinning effect



³ The implied elasticity of 1.6 is well above the range of 0.2-0.9 that Layard et al (1991) established in previous studies internationally. In Carling (1996) two of the same authors reported elasticity as low as -0,06 for the compensation level for Sweden. The 0,6 elasticity (not quite statistically significant) that Harkman (1997) found in his study of the 1993 cut of the compensation level from 90 to 80 percent is not put into the context of “Swedish evidence in the 90.s”.

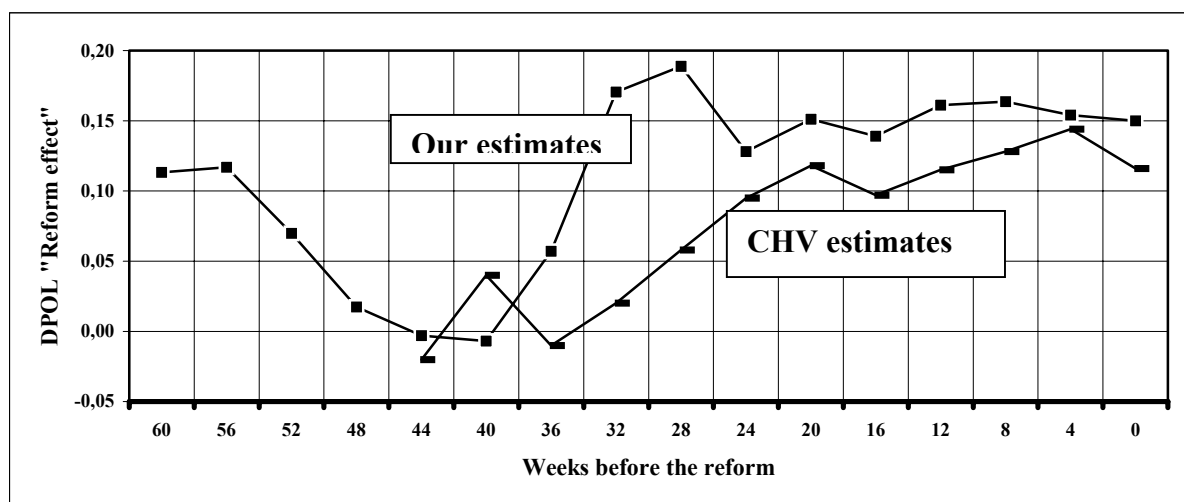
In *Figure 1* the line ABC illustrates the job finding rate at a given compensation level in unemployment insurance. At B the compensation level is lowered, increasing the job finding rate by x per cent up to the line DE. This increase is identified as the reform effect in a simple model without an anticipatory effect.

The anticipatory effect in the CHV paper is measured by moving the reform backwards in time from January 1 four weeks at a time. This method means, however, that there is a gradual thinning of the reform effect not noted by CHV.

The reform effect is an average difference of change in unemployment duration between the after-period and the before-period. The after-period average difference-in-differences is made successively thinner when it is mixed with the before-period as the reform is successively moved back in time. In *Figure 2* the drawn line AD represents the successive “dilution” of the reform effect that comes about when the reform is moved backwards. If there is an anticipatory effect, it should come out similar to the dotted line above the drawn line.

For an anticipatory effect to be statistically significant the difference between the dotted line and the drawn line must be much larger and more systematic than is documented by CHV. Their claim to have identified a statistically significant anticipatory effect is mistaken because they neglect the “dilution” effect!

Figure 3. The coefficient for the “reform effect” when the reform is successively moved backwards in time with our estimates and those represented in Figure 6 in CHV.



When we now turn to real data as reported by CHV in their Figure 6 we find our estimates to be rather different from theirs. In our *Figure 3* we have extended the simulated alternative reforms backwards by 60 weeks. Both the CHV curve and our curve illustrate statistically significant coefficients at several points in time before the reform was implemented. This is more pronounced in our estimates than in the CHV estimates.

The first significant coefficient estimated by CHV is for August 1995, five months before the reform is implemented. We estimate significant coefficients for the reform effect even for June and May before the reform bill was passed in Parliament but one month after it was announced in April. The fatal blow to CHV's interpretation of these coefficients as indications of anticipatory adaptations to the benefit cut (let alone the dilution effect) is that we estimate significant coefficients for DPOL also 56 and 60 weeks prior to the reform. This would actually be long before the reform was even announced by the government.

7. Discussion

So far we have proved that the conclusions drawn by CHV that the 1996 benefit cut in the Swedish unemployment insurance caused a 10 per cent increase in job findings and anticipatory behavioral changes before the reform do not hold. In the process we have shown how their statistical design can produce artifacts, i. e. false positive results as to reform effects. The standard statistical tests and analytic procedures used by CHV do not warn for this eventuality.

We do not claim to have proved a negative result beyond doubt; i. e. that the benefit cut did not boost job findings among the unemployed. We have not attempted to exclude the possibility that some other statistical design could provide evidence for a labour supply effect among unemployed of small changes in the unemployment insurance.

However, there are at least two straightforward reasons to doubt that it is possible to find a labour supply effect of the 1996 Swedish reform; (1) the change was economically trivial to most of the affected individuals and households, and (2) the reform was implemented in an employer's market with over 10 percent of the labour force out of work.

From tax reform studies we know that the labor supply response to even huge cuts in the income tax rates is surprisingly weak for a variety of

reasons⁴. In this case a 5-percentage cut in the fairly generous benefit level is supposed to have caused the job finding rate among those effected to jump substantially by affecting search intensity and reservation wages. One should note that a third of the benefit cut is counteracted by lower taxes in the Swedish system. The bite of the cut is further reduced by increases in income related housing allowances and reductions in day care costs for families when insurance income is reduced.⁵ It is not clear why the response would be even noticeable when a £40 daily after-tax benefit is cut to £39. The way to proving/disproving an effect may not go via a more sophisticated statistical design for the difference-in-difference approach but by evidence that substantiate the changes in search intensity and reservation wages that is theoretically assumed in search theory.

However, even if this small change in benefit affects search behavior, there is no guarantee that the job finding rate can increase much for those affected by the cut, particularly if there are few vacancies and many unemployed, as was the situation in Sweden around 1996. Where is the employer in the process, the guy who decides who is hired?

When vacancies are very few in relation to the number of unemployed looking for work, the unemployed can be seen as queuing in front of the hiring employer. The unemployed can try to get ahead towards the front of the queue or he can slip backwards or freely let others pass. The change induced by the benefit cut could be seen as a change in queuing behavior.

One would expect that those without unemployment insurance would try hard to get up in front while those with insurance would not need to behave out of line unless one's household economy is already in the red or has no margin left.⁶ If the benefit level is lowered substantially for the insured one would expect that those with the lowered benefits would forge ahead being

⁴ Swedish studies of labour supply effects for men give elasticities around 0.1 for prime age men and a little higher for women, as reviewed in Agell et al (1995). Blomquist et al (2001) find a net increase of about 2 per cent in average desired hours of married prime-age males comparing the 1980 system with 1991. 1980 is the year with historically high marginal tax rates and 1991 the year with the lowest rates in the last decades.

⁵ Selén (2002) uses the Swedish Ministry of Finance tax simulation model and data for 1995. His calculations indicate that the five percentage points cut in the unemployment insurance affected the disposable income of the unemployed, who were exposed to the 5-percent benefit cut, by only 1.2 per cent on average because of the various compensations. Even for those with the longest unemployment spells the 5-percentage point cut was reduced to only a 3.4 percent lowering of the disposable income.

⁶ The job finding rate is indeed higher and episode duration shorter among the non-insured controlling for demographic and human capital differences.

less willing to let others pass. They may be able to get ahead in the queue of jobseekers but will that necessarily increase the job finding rate?

When vacancies are scarce in relation to the number of unemployed looking for work, it is the employers' market and the employer's criteria that determine who gets the vacant job. The employer with a vacancy is likely to select the potentially most productive for the vacant job, not necessarily the most energetic job seeker or the one with the lowest reservation wage.

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Table A1. Estimates and p-values for different exit-codes from unemployment to work

| | Estimates: | | | | | | p-values according to asymptotic standard errors: | | | | | |
|----------------------------------|------------|--------|--------|--------|--------|--------|--|-------|-------|-------|-------|--------|
| Exit-codes → | 1 | 2 | 3 | 6 | 11 | 123611 | 1 | 2 | 3 | 6 | 11 | 123611 |
| Reformeffect | -0,006 | 0,033 | -0,187 | 0,223 | 0,027 | 0,150 | 0,947 | 0,836 | 0,372 | 0,183 | 0,754 | 0,004 |
| Design variables: | | | | | | | | | | | | |
| Reform (after jan 1996) | -0,430 | -0,021 | -0,079 | -0,364 | 0,184 | -0,184 | 0,000 | 0,864 | 0,600 | 0,012 | 0,016 | 0,000 |
| Testgroup with 5 percentage cut | -0,366 | -0,205 | 0,027 | -0,188 | 0,180 | -0,157 | 0,000 | 0,039 | 0,785 | 0,088 | 0,002 | 0,000 |
| Testgroup with 1-4 % cut | -0,030 | 0,052 | 0,352 | -0,216 | 0,061 | 0,009 | 0,619 | 0,672 | 0,004 | 0,164 | 0,417 | 0,815 |
| Demographic variables: | | | | | | | | | | | | |
| Sex (momen) | -0,650 | -0,547 | -1,183 | -0,454 | 0,355 | -0,240 | 0,000 | 0,000 | 0,000 | 0,000 | 0,000 | 0,000 |
| Age in years in 1995 | -0,012 | 0,000 | 0,004 | -0,035 | -0,005 | -0,008 | 0,000 | 0,991 | 0,482 | 0,000 | 0,048 | 0,000 |
| Citizenship: | | | | | | | | | | | | |
| Nordic (not Swedish) | -0,145 | -0,070 | -0,244 | 0,059 | 0,016 | -0,067 | 0,231 | 0,773 | 0,388 | 0,780 | 0,881 | 0,328 |
| Non-Nordic | -0,680 | -0,879 | -0,732 | 0,060 | -0,449 | -0,482 | 0,000 | 0,003 | 0,031 | 0,684 | 0,000 | 0,000 |
| Marital status: | | | | | | | | | | | | |
| Cohabitant | 0,117 | -0,152 | 0,148 | -0,277 | 0,063 | 0,045 | 0,042 | 0,194 | 0,231 | 0,031 | 0,243 | 0,191 |
| Married | 0,253 | 0,175 | 0,381 | -0,182 | 0,050 | 0,123 | 0,000 | 0,063 | 0,001 | 0,047 | 0,220 | 0,000 |
| Children: | | | | | | | | | | | | |
| 15 years old or younger | -0,268 | -0,441 | -0,285 | -0,106 | -0,130 | -0,199 | 0,000 | 0,000 | 0,003 | 0,245 | 0,001 | 0,000 |
| 16 years or older | 0,222 | 0,038 | 0,125 | -0,270 | 0,144 | 0,146 | 0,001 | 0,785 | 0,356 | 0,134 | 0,028 | 0,000 |
| Education: | | | | | | | | | | | | |
| 9 years or less | -0,185 | 0,113 | 0,929 | -0,485 | 0,140 | -0,049 | 0,398 | 0,825 | 0,194 | 0,079 | 0,552 | 0,706 |
| 2 years in high school | -0,052 | 0,242 | 0,738 | -0,510 | 0,359 | 0,071 | 0,812 | 0,635 | 0,302 | 0,063 | 0,125 | 0,585 |
| 3 years in high school | -0,002 | 0,170 | 0,448 | -0,692 | 0,267 | 0,009 | 0,993 | 0,742 | 0,536 | 0,016 | 0,260 | 0,948 |
| 1-3 years of university | 0,028 | 0,180 | 0,169 | -0,364 | 0,332 | 0,055 | 0,899 | 0,728 | 0,817 | 0,205 | 0,162 | 0,681 |
| 4 or more years of university | 0,559 | 0,571 | 0,578 | -0,316 | 0,246 | 0,296 | 0,012 | 0,275 | 0,434 | 0,291 | 0,311 | 0,028 |
| Experience in the wanted job: | | | | | | | | | | | | |
| None | 0,444 | 0,701 | 0,938 | -0,072 | 0,615 | 0,453 | 0,009 | 0,078 | 0,073 | 0,734 | 0,000 | 0,000 |
| Some | 0,626 | 0,983 | 1,261 | -0,052 | 0,833 | 0,652 | 0,000 | 0,011 | 0,013 | 0,794 | 0,000 | 0,000 |
| Long | 0,803 | 1,007 | 1,432 | 0,030 | 0,879 | 0,752 | 0,000 | 0,008 | 0,004 | 0,877 | 0,000 | 0,000 |
| Local labour market development: | | | | | | | | | | | | |
| Unemployment rate | 0,029 | -0,704 | 7,348 | 3,492 | -3,765 | -0,652 | 0,970 | 0,673 | 0,000 | 0,029 | 0,000 | 0,162 |
| Dummies for region and quarter | Yes | Yes | Yes | Yes | Yes | Yes | Exit code 1 = to regular permanent job Exit code 2 = to regular temporary job Exit code 3 = to previous employer Exit code 6 = to job but type of job not known Exit code 11=to other kinds of job | | | | | |
| Per cent censored out of 18 429 | 81,8 | 95,5 | 96,1 | 95,6 | 79,8 | 48,8 | | | | | | |
| -2 log likelihood | | | | | | | | | | | | |
| With covariates | 59500 | 14317 | 12406 | 13162 | 66771 | 168547 | | | | | | |
| Without covariates | 60749 | 14776 | 13223 | 13443 | 67388 | 169579 | | | | | | |

| Table A2. Estimates and p-values for different combinations of exit-codes from unemployment to "work" | | | | | | | | | | | | | |
|---|------------|--------|--------|--------|---------|--------|--|-------|-------|-------|---------|-------|--|
| | Estimates: | | | | | | p-values according to asymptotic standard errors: | | | | | | |
| Exit codes ➡ | 1 | 1+2 | 12+3 | 123+6 | 1236+11 | 1+11 | 1 | 1+2 | 12+3 | 123+6 | 1236+11 | 1+11 | |
| Reform effect | -0,006 | 0,018 | -0,009 | 0,080 | 0,150 | 0,173 | 0,947 | 0,821 | 0,906 | 0,230 | 0,004 | 0,004 | |
| Design variables: | | | | | | | | | | | | | |
| Reform (after Jan 1996) | -0,430 | -0,342 | -0,308 | -0,342 | -0,184 | -0,185 | 0,000 | 0,000 | 0,000 | 0,000 | 0,000 | 0,000 | |
| Testgroup with 5 % cut | -0,366 | -0,336 | -0,287 | -0,290 | -0,157 | -0,160 | 0,000 | 0,000 | 0,000 | 0,000 | 0,000 | 0,000 | |
| Testgroup with 1-4 % cut | -0,030 | -0,015 | 0,043 | 0,010 | 0,009 | -0,019 | 0,619 | 0,778 | 0,386 | 0,840 | 0,815 | 0,691 | |
| Demographic variables: | | | | | | | | | | | | | |
| Sex (women) | -0,650 | -0,629 | -0,701 | -0,660 | -0,240 | -0,104 | 0,000 | 0,000 | 0,000 | 0,000 | 0,000 | 0,000 | |
| Age in years in 1995 | -0,012 | -0,009 | -0,007 | -0,011 | -0,008 | -0,008 | 0,000 | 0,000 | 0,001 | 0,000 | 0,000 | 0,000 | |
| Citizenship: | | | | | | | | | | | | | |
| Nordic (not Swedish) | -0,145 | -0,135 | -0,139 | -0,112 | -0,067 | -0,067 | 0,231 | 0,213 | 0,167 | 0,219 | 0,328 | 0,396 | |
| Non-Nordic | -0,680 | -0,715 | -0,721 | -0,494 | -0,482 | -0,567 | 0,000 | 0,000 | 0,000 | 0,000 | 0,000 | 0,000 | |
| Marital status: | | | | | | | | | | | | | |
| Cohabitant | 0,117 | 0,061 | 0,075 | 0,022 | 0,045 | 0,093 | 0,042 | 0,240 | 0,118 | 0,615 | 0,191 | 0,019 | |
| Married | 0,253 | 0,237 | 0,258 | 0,192 | 0,123 | 0,132 | 0,000 | 0,000 | 0,000 | 0,000 | 0,000 | 0,000 | |
| Children: | | | | | | | | | | | | | |
| 15 years old or younger | -0,268 | -0,305 | -0,303 | -0,274 | -0,199 | -0,175 | 0,000 | 0,000 | 0,000 | 0,000 | 0,000 | 0,000 | |
| 16 years or older | 0,222 | 0,184 | 0,178 | 0,140 | 0,146 | 0,186 | 0,001 | 0,002 | 0,001 | 0,007 | 0,000 | 0,000 | |
| Education: | | | | | | | | | | | | | |
| 9 years or less | -0,185 | -0,127 | 0,002 | -0,143 | -0,049 | -0,027 | 0,398 | 0,527 | 0,990 | 0,364 | 0,706 | 0,867 | |
| 2 years of high school | -0,052 | 0,005 | 0,075 | -0,097 | 0,071 | 0,160 | 0,812 | 0,979 | 0,698 | 0,537 | 0,585 | 0,316 | |
| 3 years of high school | -0,002 | 0,031 | 0,059 | -0,140 | 0,009 | 0,133 | 0,993 | 0,880 | 0,763 | 0,378 | 0,948 | 0,410 | |
| 1-3 years of university | 0,028 | 0,062 | 0,061 | -0,099 | 0,055 | 0,176 | 0,899 | 0,763 | 0,756 | 0,535 | 0,681 | 0,278 | |
| 4 or more years of university | 0,559 | 0,569 | 0,574 | 0,341 | 0,296 | 0,417 | 0,012 | 0,005 | 0,004 | 0,034 | 0,028 | 0,011 | |
| Experience in the wanted job: | | | | | | | | | | | | | |
| None | 0,444 | 0,486 | 0,532 | 0,339 | 0,453 | 0,538 | 0,009 | 0,002 | 0,000 | 0,005 | 0,000 | 0,000 | |
| Some | 0,626 | 0,688 | 0,750 | 0,514 | 0,652 | 0,745 | 0,000 | 0,000 | 0,000 | 0,000 | 0,000 | 0,000 | |
| Long | 0,803 | 0,834 | 0,898 | 0,656 | 0,752 | 0,846 | 0,000 | 0,000 | 0,000 | 0,000 | 0,000 | 0,000 | |
| Local labor market development: | | | | | | | | | | | | | |
| Unemployment rate | 0,029 | -0,017 | 1,082 | 1,444 | -0,652 | -1,959 | 0,970 | 0,981 | 0,098 | 0,017 | 0,162 | 0,000 | |
| Dummies for region & quarter | Yes | Yes | Yes | Yes | Yes | Yes | Exit code 1 = to regular permanent job Exit code 2 = to regular temporary job Exit code 3 = to previous employer Exit code 6 = to job but type of job not known Exit code 11=to other kinds of job | | | | | | |
| Per cent censored out of 18 42 | 81,8 | 77,3 | 73,4 | 69,1 | 48,8 | 61,6 | | | | | | | |
| -2 log likelihood: | | | | | | | | | | | | | |
| With covariates | 59500 | 74074 | 86908 | 100478 | 168547 | 127422 | | | | | | | |
| Without covariates | 60749 | 75525 | 88747 | 102191 | 169579 | 128137 | | | | | | | |

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